

the process. Therefore do not dismiss this as the sketch of a fancy battle. Watch for yourselves; place within the pipe at the back of the mouth some fine filaments of cotton, or fluff or down; advance them from the interior to the inner edge of the windway, and you will see them shot with energy not upward into the pipe, but outward full in your face with an unmistakable trajectory. Do we not bring into activity the same force, "suction by velocity," when we blow through one little tube over another tube leading down to a well of perfume and draw up thereby scent-laden globules caught in the belt of wind passing over the tube's orifice, dispersing fine odour-sprays into the atmosphere? When a train of carriages loosely coupled is starting out of a railway station, should the engine suddenly back a little we see the hindermost portion of the train with its acquired momentum meeting the foremost portion advancing to it with reversed direction of impetus, and the central carriages receive a double compression, a rude kind of node is thus formed starting a reaction of bufferage in opposite directions; so when trains come into collision or are suddenly stopped in career, the distribution of weight, the gradients and relative velocities determine which portion feels most the influence of the shock. Again an analogy. There is a country custom, when the bees swarm to dredge them with flour as a means of identification, if the flour *travels* you will know the bees have journeyed likewise. Take a piece of white tissue paper (a bank-note answers it admirably), fold it so that a portion will occupy very nearly the space of the embouchure of the diaphanous pipe, by using a card it may be held level on the outer edge of the windway, it is in fact a paper reed but flaccid and inanimate; as you advance it to the windway no sooner is it caught in the current than it darts upright and becomes incorporated with the air-reed,

"Grows with its growth and strengthens with its strength."

This same crisp little bit of paper will reveal to your eyes the treasured secret of the organ-pipe, tell you how its wealth of varied tone is wrought, show you its fine arcs of flexure, how it bends less for its inward than for its outward stroke, and how its free curves are moulded to your will; listen, and you shall hear the domestic wrangle of the reed and pipe; look, and you shall witness how in its high caprice it transmutes in a flash to harmonic speed and leaps exultant to its octave. Truly an Ariel imprisoned, endowed with form, and clothed with a white vesture making it in all its motion visible as bees.

On the supposition that the theory herein advanced is justifiable, the work of the aeroplasmic reed is to be considered, specifically, *to abstract*. By reason of abstraction rarefaction ensues, condensation correlates therewith, the latter springing out of the former, and the product is vibration. The reed is the generator of the power and the node is the fulcrum of vibration, the place of reaction, with this peculiarity that it affords an elastic fulcrum sensitive to the encroachments of the column of air above it; in the stopped pipe on the contrary there is a stable unyielding fulcrum, and the results of this difference are very remarkable, as will be seen in another paper necessary to complete this exposition, but at present I can only allude in passing to one of these results which it seems desirable not to omit here. Admitting my affirmations so far as they can be proved by other eyes, objections will be taken to the imaginary description of the action of air-particles and waves in the interior of the pipe, as opposed to received doctrine. Novelty is often held to be outrage. It is an essential feature of my hypothesis that the initial movement, or prelude to vibration in the pipe, is distinct from successive movements both in its course and character; it extends throughout the pipe, is continuous but diminishing in degree, and is without a node, which is only fully established at the second course. Without entering now into further details it is important to notice that this interval between the first effort or gasp of the pipe and the full possession of its power, is distinctly perceived by the ear. All musicians acquainted with organs are conscious of this, and it is matter of usual comment with them how that stopped pipes are on the contrary remarkably quick of speech, instantaneous in articulation. They feel this without reasoning of why or wherefore. As in stopped pipes there is no supernodal column, no requirement for an effort similar to that awakening motion to perfect vibration in open organ-pipes, the verdict of the ear is in both cases consistent with and corroborative of the hypothesis. Experiments with a very peculiar pipe called the "German Gamba" will throw invaluable light on the process of tone-making in organ-pipes.

HERMANN SMITH

The Degeneracy of Man

WITH regard to the culture of savages in Brazil the evidence of facts will be more esteemed by Mr. Tylor than the opinion of Dr. Martius, for Mr. Tylor has brought together a wealth of facts on the history and conditions of culture.

There is one class of facts which to my mind bears particularly on this question of the tribes of Brazil and the Amazons, and that is language.

The Kiriri and Sabuyah of Bahia as also the Ge have affinities with the Shoshoni and other dialects of the Rocky Mountains, and it is difficult to believe a language of this kind can belong to an epoch of high culture.

The dialects of the Tocautius have affinities of a like character with the Ankaras and Wun of Africa, and with that of the Akka pigmies just discovered in the Nile region.

The Purus, Coroado, and Corope of Rio Janeiro appear to belong to the Carib directly, and thereby also to Africa.

In the present state of our materials and information it is impossible to define exactly the members of each class. Thus the two groups last mentioned appear to be connected by the Baniwa and the Carib.

The main body of the population of Guarani, Tupi, Omagua, have by me been long since pointed out as having a language similar in roots and grammar to the Agaw of the Nile region. This is the highest development of language known to me in Brazil.

If the tribes of Brazil have fallen from a higher estate it is strange they should have become endowed with languages of the Prehistoric epoch.

HYDE CLARKE

June 29

THE gradual degeneracy of savage man from a higher type is a fact which an eminent author states in his letter in NATURE (vol. x. p. 146) to be difficult of belief. He wonders that Dr. Martius should say "the Americans are not a wild race, they are a race run wild and degraded."

The following facts seem to me to support the view held by Dr. Martius, Alex. von Humboldt, Abp. Whately, the Duke of Argyll, and others.

In the Ilium now laid bare by Dr. Schliemann, the lower strata contain more copper and fewer stone implements than the upper. "In other words, we have the very 'unscientific' fact of an 'age of stone' above an 'age of copper'" (Quart. Rev., April 1874). "The newly opened mound of Hissarlik stands as a lasting witness to a progressive decay of civilisation, industry, and wealth, among the successive races of its inhabitants" (Quart. Rev.).

Among the forest tribes of Brazil Dr. Martius found traces of the village community with its tribe-land common to all, while huts and patches of tilled ground were treated as acquired property, the recognised owners not being individuals but families. This may be well explained as a custom brought by Asiatic immigrants into the American continent. The Chinese anciently divided the land of a village into nine parts. The division was made by two perpendicular and two horizontal parallel lines. The middle square was common land. The eight remaining squares were assigned to eight heads of families, who cultivated the common square, giving the produce to the Government: they constituted a village. This principle of revenue collection based on land distribution existed for many centuries in ancient China, and was afterwards changed for a grain tax in kind about the time of the building of the Great Wall. Agricultural emigrants to America at any date before 200 B.C. would be familiar with this mode of doing things, and would naturally carry with them the knowledge of this and other customs existing at the time in eastern Asia. The appearance of a principle of land distribution resembling that of the old Teutons, among American tribes, cannot then be taken as proof that they were progressing and not degenerating, for it may, when our knowledge of ancient America becomes more accurate, be seen to be one of the lingering remains of an older civilisation which in a tropical climate favourable to indolence would easily decline. The religious beliefs and social customs of Asiatic and American races are in many respects so similar that there is abundant ground for questioning the originality of any civilised custom found among American tribes. Why should not comparative ethnology one day find the key to solve all such questions?

This fact, looked at from the eastern Asiatic point of view, is so far then from supporting the theory of progressive development, that it may rather be used as an additional buttress for the theory of degeneracy.

Names of number among Malayan and Polynesian tribes may be referred to as a proof of degeneracy. The sound "man" is 10,000 among the natives of Samoa and Tonga, as it is in Chinese, but it is 4,000 in the Sandwich islands, and 1,000 in New Zealand. Islanders avoid high numbers, and allow the significance of a name of high numbers to sink. This is proof of degradation. The reason why the arithmetical faculty among the New Zealanders has become weaker than elsewhere is because of their enormous distance from the continent of Asia. Samoa and Tonga are much nearer, and accordingly in those islands the religious traditions, e.g. circumcision, resemble those of Asia very closely. The Polynesians formerly had a decimal arithmetic, now it has sunk in Australia to quaternary or quinary arithmetic. In Ponape, one of the Caroline group, and comparatively near to the continent, *apiki* is 100 of men, trees, or yams, but 1,000 of eggs, cocoanuts, or stones. In Chinese *pak* is 100. After centuries of use high numbers fluctuate in value, because the intellect of islanders declines in power as the effect of long-continued isolation. The ideas, names, and usages of civilisation are gradually lost, and with them the human intellect becomes dwarfed.

Prof. F. Müller, after showing that the Polynesians could originally count to 100, adds, "Dies ist gewiss ein Zeugnis für die nicht geringe geistige Begabung und frühzeitige Entwicklung dieser Völker." * The Polynesians, then, have sunk in power, and were, when visited by Capt. Cook, in a state of progressive degradation.

The question raised by Mr. Tylor was only—"Did Dr. Martins change his opinion about the degeneracy of Brazilian tribes?" Dr. Peschel thinks he did, but has not yet given sufficient proof. While I venture to think that the question—"Is savage man a degenerated being?" can be solved in the affirmative by the careful comparison of facts, without our needing to know that each scientific traveller holds this view, it would be most interesting to be assured that all such men are agreed upon it.

JOSEPH EDKINS

Disuse as a Reducing Cause in Species

In a letter of mine (*NATURE*, vol. ix. p. 361), entitled "Natural Selection and Dysteleology," there occurs a footnote upon the above subject. As this footnote was rather carelessly written, I wish to explain my meaning more clearly.

In the first place, it is evident that the fact of disuse causing atrophy in individuals is no proof that it likewise causes atrophy in species; for if it does so, the laws under which it operates in the two cases must be quite different—the one set being as exclusively related to Inheritance, as the other set are independent of this principle. The primary question therefore is: Does inheritance here reproduce the character of immediate ancestors, as in congenital atrophy, &c.; or of distant ancestors, as in mutilations, &c.? I think there can be no reasonable question that it does the former, and so have no doubt that disuse is a cause of atrophy in species. The question as to degree, however, remains.

One sentence in the footnote I am explaining may be taken to imply that the effects of disuse are exhausted in a few generations. Nothing can be further from my meaning. If disuse acts at all in species, its *modus operandi*, as just stated, must be that of causing variations which are capable of being inherited; consequently, if disuse acts thus at all, it is impossible to assign limits to its operation in time. The question, however, is, In what proportion are the effects of disuse in the parents reproduced in the offspring? Variations caused by disuse certainly differ from congenital variations, in that they are not fully inherited; and it is the degree in which they are inherited that must determine the rate at which disuse here operates. This degree, however, is unknown: we only know that it is something very small. Now as disuse is in competition with other reducing causes, the rapidity of its action is an important factor in the estimation of its probable effects.

By the omission of the word "proportional" near the end of the footnote, I appear to institute an absolute comparison between the effects of disuse in wild and in tame species. This, of course, would be absurd. What I mean is, that supposing disuse to be the chief cause of atrophy in wild species, it has not produced so much effect in tame species as we should antecedently expect; for, although the facts are very scanty, so far as they go they tend to prove, that when an organ is disused for several generations only, the rate of its reduction is much greater than it ought to be, supposing disuse to be the main

cause of atrophy in our domestic animals, and supposing the action of this cause to be uniform.

It will be asked, If we thus in part reject this cause, what other have we to substitute? This, of course, is a collateral issue; but as it is an important one, it may here be discussed. I would suggest the cessation of selection (see *NATURE*, vol. ix. p. 440) as a co-operating cause, for it seems to me that this *must* have acted here to some extent, and if no other causes have been at work, this extent must be the complement of the effects due to disuse. For the sake of definition, therefore, we shall assume disuse to be in abeyance. Now, on this assumption, we should expect to find that atrophy proceeds more rapidly during the initial stages of reduction than subsequently. But without dwelling upon this point, what may we infer from the existing degree of atrophy in the affected organs of our domestic animals? Supposing the cessation of selection to be the only cause at work, what degree of atrophy should we here expect to find? Before I turned to the valuable measurements given in the "Variation," I concluded (Cf. *NATURE*, vol. ix. p. 441) that from 20 to 25 per cent. is the maximum of reduction we should expect this unassisted principle to accomplish, in the case of natural as distinguished from artificially-bred organs. Now on calculating the average afforded by each of Mr. Darwin's tables, and then reducing the averages to parts of 100, I find that the highest average decrease is 16 per cent., and the lowest 5; the average of the averages being rather less than 12. Only four individual cases fall below 25 per cent., and of these two should be omitted (Cf. "Variations," p. 272). Thus, out of eighty-three examples, only two fall below the lowest average expected. Moreover, we should scarcely expect disuse alone to affect in so similar a degree such widely different tissues as are brain and muscle. The deformity of the sternum in fowls also points to the cessation of selection rather than to disuse. Further, the fact that several of our domestic animals have not varied at all is inexplicable upon the one supposition, while it affords no difficulty to the other. We have seen that disuse can only act by causing variations; and so we can see no reason why, if it acts upon a duck, it should not also act upon a goose. But the cessation of selection depends upon variations being supplied to it; and so, if from any reason a specific type does not vary, this principle cannot act. Why one type should vary, and another not, is a distinct question, the difficulty of which is embodied by the one supposition, and excluded by the other. For, to say that disuse has not acted upon type A, because of its inflexible constitution, while it has acted on a closely allied type B, because of its flexible constitution, is merely to insinuate that disuse having proved itself inadequate to cause reduction in the one case, it may not have been the efficient cause of reduction in the other. But the counter-supposition altogether excludes the idea of a casual connection, and so rests upon the more ultimate fact of differential variability, as not requiring to be explained. Lastly, it is remarkable that those animals which have not suffered reduction in any part of their bodies are likewise the animals which have not varied in any other way, and conversely; for as there is no observable connection between these two peculiarities, the fact of the intimate connection between them tends to show that special reduction depends upon general variability, rather than that special variability depends upon special reducing causes.

Dropping, however, our argumentative assumption, it will be remembered that I deem it in the last degree improbable that disuse should not have assisted in reducing the unused organs of our domestic animals; and the effect of this remark is to show that the cessation of selection is not able to accomplish so much reduction as I antecedently expected. On the other hand, it seems to me no less improbable that the cessation of selection should not have here operated to some extent; but in what degree the observable effects are to be attributed to this cause, and in what degree to disuse, I shall not pretend to suggest.

No doubt the above considerations are of a very vague description; but this only follows from the scarcity of the data at our disposal, and it is to this very scarcity that I am principally desirous of calling attention; for although it is with reluctant diffidence that I venture thus, even in part, to dispute the doctrine of one whom most of living men I venerate, yet, for the reason just given, I cannot help feeling that the time has not yet arrived for a final quantitative decision upon this subject. However, as before remarked, "the question thus raised is of no practical importance; since whether or not disuse is the principal cause of atrophy in species, there is no doubt that atrophy accompanies disuse."

GEORGE J. ROMANES

* "Reise der Novara." Linguistischer Theil, 1867, p. 287.